SCIENCE:

A WEEKLY RECORD OF SCIENTIFIC PROGRESS.

JOHN MICHELS, Editor.

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To Correspondents.

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Those engaged in Scientific Research are invited to make this Journal the medium of recording their work, and facilities will be extended to those desirous of publishing original communications possessing merit.

Proceedings of Scientific Societies will be recorded, but the abstracts furnished must be signed by the Secretaries.

Both questions and answers in "Notes and Queries" should be made as brief as possible; an answer appearing to demand an elaborate reply, may be written in the form of an article.

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PROFESSOR LEIDY'S "FRESH WATER RHIZOPODS OF NORTH AMERICA."

Dr. Leidy is acknowledged to be the highest authority on the subject treated in his great work, "Fresh Water Rhizopods of North America;" a criticism of the book becomes, therefore, a work of supererogation, and we reserve to ourselves the more pleasing task of pointing out its many beauties and particularly its importance as one of the most valuable contributions to the literature of microscopic forms of life.

Published by the Department of the Interior of the United States Government, and forming volume twelve of the "Report of the United States Geological Survey of the Territories" in charge of Professor F. V. Hayden, it is produced in a sumptuous form which no private publisher would have dared to imitate.

Dr. Leidy's Report covers about three hundred folio pages, illustrated by forty-eight full sized plates, printed in colors in the highest style of lithography.

It may be a superfluous question to most of our readers, but as Dr. Leidy himself inquires in the first page of his work, "What are Rhizods?" In reply he says, "Rhizopoda are the simplest and lowest forms of animal life, constituting the first class of the Protozoa. They derive their name from the Greek word *rhiza*, a root, and *pous*, a foot. They are mostly microscopic beings, although sometimes sufficiently large to appear as conspicuous objects." We may add that the essential characters are the gelatinous structureless bodies, and the locomotive organs consisting of variable retractile root like processes (pseudo-poda or false feet).

Their minuteness is compensated for by their multitude and wide-world distribution; essentially aquatic they occur wherever there is moisture; the search for them may be commenced in the crevices of the stones at your door step, and may be continued in every marsh, pool, ditch, pond, lake, sea and ocean, and from the greatest depths of the latter to the snow lines of mountains.

The particular Rhizopods which form the subject of the book now under consideration, are those found in fresh water only, and Dr. Leidy expressly states that his attention, during the four years engaged, was directed more to the discovery and determination of the various forms occurring in the United States, than to the elaboration of details of structure, habits, modes of development, and other matters pertaining to their history.

Although it is professedly an illustrated catalogue of the fresh water Rhizopoda of North America, we find most interesting and valuable contributions to their life history which makes us regret that time and opportunity did not permit Dr. Leidy to extend his observations in this direction, for we know how exhaustive such a treatise would have been from his hands.

Instead of writing a discursive essay upon Dr. Leidy's book, already done by many brillant writers, which, with a work so purely technical, seems the least profitable method of treating the subject, we propose to take our readers through the book, acting the part of a friendly guide, trusting by the aid of twenty-two illustrations we have reproduced for this purpose, not only to do justice to the work in question, but to convey to those who have not

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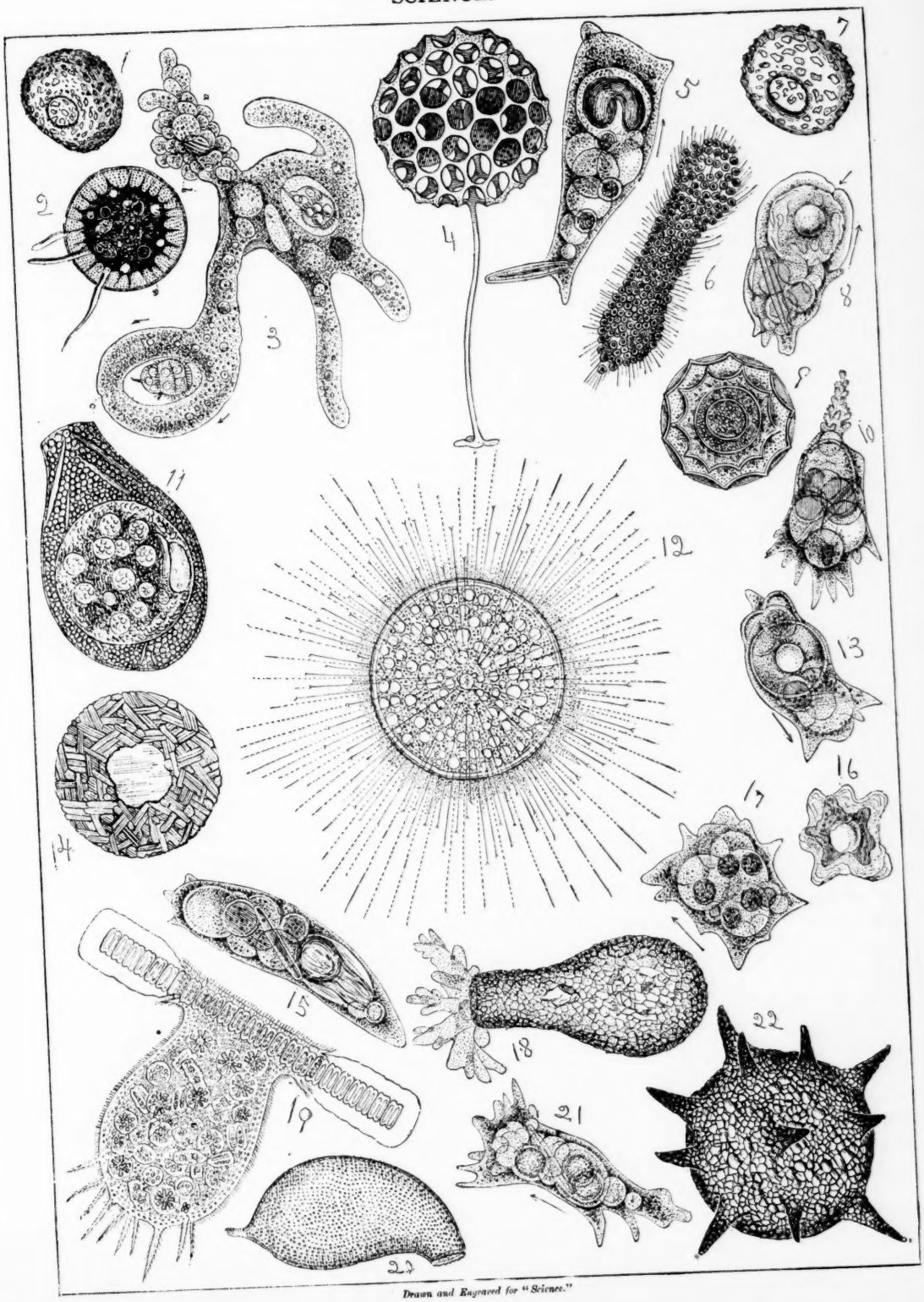

Drawn and Engraved for "Science."

studied the subject, a fair idea of the nature and form of Rhizopods.

In figure (3) we have a representation of the Amœba proteus, the most simple and wonderful of Rhizopodic forms—it is, to all appearance, a structureless gelatinous mass. As the *International Review* states (we fear somewhat rashly) it is without organs; they are certainly not permanently visible, and so far the most intelligent research has not discovered them; but the Amœba has the power of extemporizing such organs as are necessary for its existence, and has the means of reproducing its species. In the drawing, the pseudopoda, or false feet, are seen extended, and the animal is sketched while in the act of capturing an infusorial, by the act of joining the points of two of these pseudopoda.

In the Dinamœba mirabilis, figure (19), we find an advance made in structure, and a more specific form; the interior may be noticed to be full of desmids on which the animal feeds, and its posterior is widely expanded, so as to embrace a cord of Didymoprium.

In figure (18), Difflugia pyriformis, our Rhizopod is now found with a case or shell formed of irregular particles of quartz sand.

The empty shell of another of the same species, D. lobostoma, is shown at figure (14). The shell is composed of rectangular and oval plates, with dotted intervals.

Another of the Difflugia, D. Corona, at figure (22), still with a case formed of the same material, but of a somewhat different form, and having eleven spines; on the reverse side, the mouth would have been seen armed with teeth.

In figures (1) and (7), D. Constricta, we have further examples. In the original drawing they are colored a light redish-brown, whereas the previous examples were colored a blackish tint.

We now approach a beautiful design in form, the Nebela collaris, figure (11), the sarcode being contracted in the shape of a ball.

The design shown in figures (2) and (9), Arcella vulgaris, is still more beautiful and decidedly approaching the delicate patterns seen on the silicious valves of diatoms. The first in the original drawing is colorless, the second a light-brown, the sarcode being encysted.

In figure (20) is seen the empty shell of Cyphoderia ampulla, having a form somewhat similar to the Marine Rhizopods; the spur-like process may be noticed.

Clathrulina elegans, figure (4), is a very beautiful and remarkable form of Rhizopod, having a yellow colored lattice shell, enclosing sarcode in two balls, and supported by a stem of the same color. In other specimens Dr. Leidy shows the sarcode balls emitting numerous rays; this appears to be omitted in the present drawing.

At figure (6), Diplophrys Archeri, is a form of a different class taken from the swamp water on the mountains of Pennsylvania. Dr. Leidy describes them as composed of multitudes of minute globular individuals aggregated in masses, which in their movement causes the whole to undergo a change of shape. The corpuscle seen in each transparent body should be colored a bright cherry-red, to imitate the original drawing.

Acanthocystis chætophora, represented at figure (12), is very similar in form to the Actinophrys sol, or Sun animalcule, of the text books. The body is a finely granular protoplasm, invested with numerous delicate, silicious rays, implanted by minute basal disks; there are also numerous soft rays, like those of the Actinophrys, but distinguished from the silicious rays by the former ending in a simple pointed or furcate extremity.

We have now carried our readers briefly through Dr. Leidy's book, and have shown the beauty and variety of the forms studied. The interest attached to such investigations is obvious, and they are within the reach of every student. Considerable work has still to be done in this direction, and however great may be that done by Dr. Leidy, we feel sure that he would be the first to admit that so far the ground has been but broken.

To our mind it seems most important to concentrate future work on the genus Amœba, for therein lies the nucleus of the most valuable discoveries, which may even be of the highest importance to the human race. What is required in this direction is the constant and continuous observation of a single individual of the species, so as to arrive at its life history. We did hope that Dr. Leidy, in his present work, would have added to our knowledge on this point; but he frankly admits that neither himself nor others profess much knowledge relating to the reproduction of the Amœba.

It is, however, a question with us, if Dr. Leidy has not missed such an opportunity during his recent investigations.

At page 49 of this work on Rhizopods we notice Dr. Leidy describes what he calls an act of cannibalism, when he saw an individual (Amœba proteus) swallow and digest one of another species (Amœba verrucosa). We would ask, was this really an ingestive or a sexual act? Dr. Leidy takes the former view. With great respect for his

experience, we suggest that it may have been the latter, and we do so on the following ground.

In 1873 the Rev. W. H. Dallinger, F. R. M. S.,

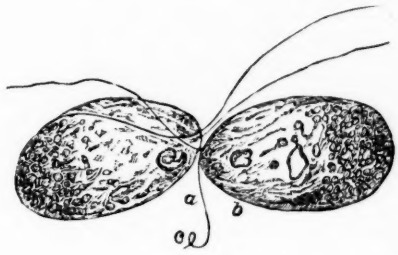

FIG. 1.

undertook, with Dr. J. Drysdale, to make a continuous microscopical investigation into the life history of a certain species of Monas. For this purpose they constructed apparatus which prevented the evaporation of the water, and the conditions were made perfect, to keep the specimen in a living state for a considerable time under the microscope. They then commenced to watch the monads in turns continuously, keeping the object in view without a break in observations for many days. During this close study of these monads, Messrs. Dallinger and Drysdale also observed an act so similar to that noticed by Dr. Leidy, that we will produce both statements for comparison.

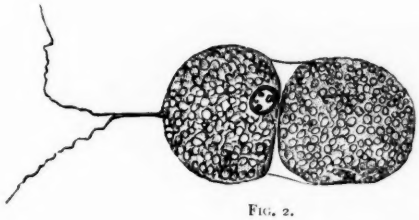

FIG. 2.

Messrs. Dallinger and Drysdale found the modes of re-production with the monads to be equally as varied as that known to exist with the Amœba, but there was one method which particularly attracted their attention.

Two monads at times met and touched their anterior ends, swimming freely together (figure 1), their bodies *then melted into each other*, (figure 2); it then became a single oblong mass, the line of juncture slowly disappearing, when after from six to 24 hours, it became rounded, (figure 3); at length the edges gave way, and myriads of minute points poured out, which were watched until they developed into perfect monads.

In figures Nos. (5, 8, 10, 13, 15, 16, 17, 21)* of our illustrations may be seen in the drawings, from Dr. Leidy's work, of what he observed of the mingling of two Amœbæ; he first speaks of their mutual approach, followed by an "embrace," when the jointed ends of the pseudopods *fused together*, and one sank deep into the body of the other, and eventually assumed the appearance of a sphere; further internal breaking up was then noticed, but later observation was not made. This is to be regretted as it seems quite probable that as "*the melting into each other*" was proved in the case of the monads to be a sexual act, that the *fusion*, when noticed by Dr. Leidy in the Amœba, may have been of the same character.

The necessity for the continuous mode of study

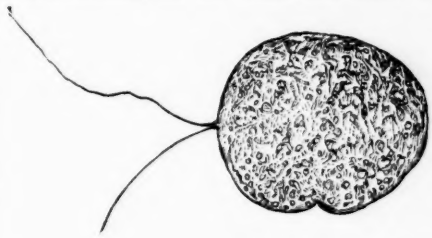

FIG. 3.

of such forms becomes more apparent every day, especially in the case of the Amœba, for in the very next paragraph to that just quoted, Dr. Leidy states "in my studies of Amœba proteus I have not been so fortunate as to trace its history from an early period, nor to discover its modes of reproduction."

There are other points to which we would refer, but having already exceeded our limit of space, we can only conclude by extending our thanks to Dr. Leidy for this his latest and most valuable contribution to SCIENCE, and to those who had the foresight and judgment to direct its publication.

* Figures (5, 8, 10, 13, 15, 16, 17, 21) illustrate the series of changes observed in the swallowing and digestion of an *Amœba verrucosa* by an *A. proteus*. From Bristol Marsh, Pennsylvania, August 27, 1876, 500 diameters. Fig. (16). *Amœba verrucosa*, comparatively quiescent with central contractile vesicle. Figs. (5, 8, 10, 13, 15, 17, 21). Successive changes in shape and relative position of *A. proteus* during the act of swallowing and digesting the former. Fig. (21). *A. proteus* approaching the *A. verrucosa* with anterior short diverging pseudopods. Fig. (8). The *A. proteus* embracing closely the *A. verrucosa* by a pair of digitate pseudopods, the points of contact of which being marked by the left hand arrow. Fig. (13). The *A. verrucosa* swallowed and forming a large sphere within the *A. proteus*. Fig. (10). The *A. verrucosa*, within the latter, has assumed an oval form, and is contained within a vacuole. The central contractile vesicle, which until now had remained persistent, had become less distinct. Fig. (15). The *A. verrucosa* has assumed a pyriform shape within a large elliptical vacuole and its contractile vesicle disappeared. Fig. (5). The *A. proteus* in the act of discharging a diatom, while the *A. verrucosa* has become doubled on itself. Fig. (17). The remains of the *A. verrucosa* seen as five granular balls within the *A. proteus*. Later these balls disappeared and their material appeared to be diffused among the granular contents of the *A. proteus*.

THE FOURTH STATE OF MATTER.

A REFUTATION.

Translated for "SCIENCE" *by Gustave Glaser, Phil. D.*

It may interest the readers of "SCIENCE" to know the opinion held in Germany respecting those phenomena which led Mr. W. Crookes to believe he had discovered a fourth state of matter. For this purpose we have translated and abridged an article by Dr. J. Puluj, the well-known scientist of Vienna, published in the "*Chemiker Zeitung*:"

According to Mr. Puluj, the beautiful experiments of W. Hittorf published in 1869, under the title "Electrical Conductibility of Gases," have received too little attention from our scientists, it may be, on account of the modest title. The scientific labors of Goldstein, and some interesting researches of Reitinger and Urbanitzky have met with the same fate. W. Crookes, the renowned English chemist, to whom the writings of the above-named gentlemen were evidently unknown, made similar experiments, the results of which did not differ essentially from those of Mr. Hittorf. His conclusions were, however, entirely new; he declared that his experiments proved a fourth state of matter.

The conception was daring, still more daring the hopes which he and his friends based upon the discovery of "radiant matter." The cause of these high expectations is the following: When an electrical inductive current is led through a molten glass tube in which the air is attenuated to $\frac{1}{1000}$ of its density, there appears on the negative pole a blue (glimmering) light, which is separated by a dark space from the cluster of light at the positive pole. If a greater attenuation takes place, the cluster of light disappears and the glimmering light floats over the whole tube, while at the same time, next the electrode a second dark space appears which becomes greater with the greater attenuation. If the attenuation still further increases, the dark space fills the whole tube and the glass walls shine in a brilliant, green, phosphorescent light. Mr. Crookes now believes that this phenomenon of phosphorescence comes from the remaining gas, which at this high state of attenuation has passed into an ultra-gaseous state, a "fourth state of matter."

But these phenomena are very different at a higher pressure. Direct measurements have shown that the phosphorescence does not appear at the millionth attenuation, and that the thirty thousandth attenuation is sufficient to produce it. Besides, this attenuated gas retains its characteristic properties, which could not be the case if by this attenuation it became dissolved into the original molecules which form, as Mr. Crookes says, the basis of all.

That the physical properties of this remaining matter are not changed, but remain in strict accordance with the kinetic theory of gases, also proves that we have no new state, but simply a gaseous state of matter. For example, the above-mentioned phenomena, in experimenting with the lighter gases, are visible at a lower altenuation than in experimenting with the heavier gases. The supposition of the renowned chemist, Dumas, that our elements are only chemical combinations of higher order, and complicated aggregates of primitive molecules, has, undoubtedly, much probability about it, but even the strongest electrical currents, and the highest temperatures, have not been able to produce this final dissolution of the elements, therefore it is not likely that a high attenuation can.

Dr. Puluj's experiments go to show that Mr. Crookes' so-called radiant matter "consists of negative electric particles," which are torn off from the negative electrode and hurled away with immense rapidity. These electrode particles form a very beautiful metallic mirror on the glass walls. [Aluminum particles are the only ones which form no metallic deposits. This may be accounted for by their chemical constitution.] The conduction of the current, therefore, is effected by the convection of the electrode particles, in which static electricity is accumulated. We have here a case of molecular electric convection, analogous to that observed by Mr. Rowland in his experiments. This gentleman has demonstrated that when a movable horizontally placed metal ring, charged with static, positive or negative, electricity is made to rotate around a vertical axis, it will divert a magnetic needle suspended above it, in the same manner, as if an electric positive or negative current were to move in the same direction with, or in an opposite direction from, the rotation. These experiments of Rowland lead to the inference that an infinitely small electrical globe, in our case an electrode particle, will have a similar influence upon a magnet. As long as the globe and magnet are at rest, it is to be expected that no alternate effect will appear, but that this will be produced as soon as the little globe is put into violent motion. Because the electrode particles are negative electric, they represent a positive electric current, which moves in an opposite direction from the former. The electrode particles in motion are, therefore, real elements of an electric current, and are subjected to the law of Laplace. Their deviation takes place according to the following simple law: If we imagine that a plane is placed through the direction of the motion of the electrode-particle and through the north pole of the magnet, and suppose that a man is lying upon this electrode particle in the direction of the motion, and looking towards the north pole, then the electrode-particle will be diverted towards the left hand of this man, vertical to the imagined plane. This simple law gives a sufficient explanation for all the phenomena which a magnet produces in the radiant electrode-matter, and which were observed by Mr. Crookes as well as by Mr. Reitinger and Urbanitzky. It proves that the glimmering light at the negative pole is not a "magnetic" light, but the consequence of a molecular electrical convection, and it justifies the supposition that an electrified current or vapor which is led through a tube will deviate the magnetic needle in the same manner as an electrical current going through a telegraph wire.

The law of the indestructibility of force has already solved many problems which puzzled the scientist of earlier centuries. According to the same law, we must assume that when infinitely small projectiles of radiant electrode matter are hurled against the glass walls of the tube their motion is changed into molecular motion, and the glass walls are heated by the collisions, sometimes even to the melting point; but at a lower temperature the rays which are not very much concentrated only produce a phosphorescent light of the glass.

The extremely fine matter called ether, which fills all space and pierces all bodies, surrounds the molecules, as the atmosphere surrounds our globe. Each body and each molecule has in its normal state a certain quantity of this ether. When this quantity is greater than the normal quantity, the molecules, according to the "unitarian view" of elasticity, are positive electric; when it is smaller, they are negative electric.

Supposing now that a collision takes place between the negative electrode particles and the molecules of the glass walls of the tube, then the equilibrium will be restored at each point of collision and the molecules of the glass will lose their surplus of ether. At the same time a motion of the waves of ether will be observed, and this motion is felt by our optical nerves as phosphorescent light. Therefore the phosphorescence observed by W. Crookes is the result of the restoration of the ether-equilibrium and not of the heating of the glass, whose temperature during the appearance of this phenomenon is comparatively low.

At a lower degree of attenuation, the stream of electrode matter pushes back the attenuated gas, and this explains the dark space which appears in the tube. This dark space is analogous to the dark space in a gas flame, which is to be seen near the mouth of the gas tube, and is produced because the outstreaming gas pushes back the particles of air which, coming from an opposite direction, try to enter the tube.

* * * * * *

Another observation of Mr. Puluj also contradicts the conclusions of W. Crookes. Puluj has observed that, at a higher attenuation, the electrode is moving towards the aluminium side, *i. e.*, in opposite direction from that observed by W. Crookes. According to Mr. Crookes the cause of motion is a double one, the higher temperature of the electrode at the metallic side and the emission of electrode particles.

Both effects are opposite. At a lower attenuation the effect of the heat is greater, and the electrode moves in the direction of the wings of the radiometer, with the colder side ahead, at a higher attenuation, the effect of the emission of electrode particles is predominant. Radiant electrode matter and the electrode itself move in the *same* direction.

This remarkable discovery proves not only the incorrectness of Mr. Crookes' explanation, but is also in direct opposition to the principle of the preservation of the centre of gravity, which is made by Mr. Crookes the basis of his arguments.

The Vienna scientist draws from his observations the conclusion that the forces by which the electrode particles are torn off are not interior but *exterior* forces. When the electric current passes through the electrode, there is, according to his opinion, really a stream of extremely fine matter (ether) flowing, which not only tears off particles of the electrode, but also sets the whole electrode into motion.

This view seems to be a new proof of the unitarian hypothesis, which maintains that an electric current is nothing else but a current of ether.

Even if the number of scientists who follow the dualistic hypothesis of electricity is by far greater than that of the Unitarians, the view of the latter deserves at least our attention, especially when such men as Franklin, Secchi and Edlund approved it.

THE MAGNET IN MEDICINE

Translated for "SCIENCE" *by Thos. B. Columbia.*

Some recent researches made under the direction of Prof. Charot in his laboratory at the Salpêtrière have drawn attention anew to a therapeutic agent known for a long time, but to-day almost abandoned. We find, in fact, even in the works of the oldest authors, traces of attempts made by physicians to apply the magnet in the treatment of disease.[1] But the want of precise rules in its application and the appearance of mystery and of fancy which is attached to this kind of research explain the discredit into which this means of treatment has fallen.

We are indebted to Prof. Maggiorani for having undertaken, in about 1869, the restoration of magnetic therapeutics, by seeking to establish it upon rational and truly scientific principles.

It was in the train of the experiments undertaken by the Commission appointed by the Biological Society, of Paris, with the object of verifying the facts collected by M. Burq under the generic title of Metallotherapy,[2] that the first attempts toward the application of the magnet were made at the Salpêtrière. After the results obtained by the application of metals, it was natural to seek to clear up the singular phenomena by varying as much as possible the conditions of the experiment. In this way it was shown that the plates of the different metals were not the only agents capable of acting upon a certain class of diseases (neuroses, and particularly hysteria, organic affections of the cerebral nervous system). Similar results were attained with many physical agents: feeble currents, statical electricity, vibrations of sonorous bodies, differences of temperature, magnetized bars, electro-magnets, solenoids, etc. Very soon the magnetic bars were noticeable for the constancy of their action and facility of their use.

Magnets are, therefore, not endowed, from this point of view, with specific properties; they form part of a group of physical agents which, to different degrees, possess the same power of impressing the nervous system and of giving rise to biological phenomena; and although magnets are here particularly spoken of, it must not be forgotten that they are not the only ones concerned.

The status of the question has been clearly exposed by Dr. Vigoroux in the *Medical Annual* (1879). To this article I must refer those who wish to become acquainted with the *ensemble* of phenomena, which are included under the name *metalloscopic*. These studies, begun at the Salpêtrière, have given rise to active discussions. The facts announced have been confirmed, wholly or in part, in Germany by Müller of Grätz, Westphal, Vierordt, Schiff, Adamkiewicz of Berlin; Benedick of Vienna, Rumpf of Dusseldorf; in Italy, by Seppilli, Maragliani, and especially Maggiorani; in England, by Gamgee, Sigerson, H. Tuke; in France, outside of the work of the Commission, I will mention only the thesis of M. Aigre and the observations of MM. Dumontpallier, Vigouroux, Landouzy and Debove, who have verified the therapeutical action of the magnet. But the results obtained were sharply attacked on the other side of the Channel by Hughes, Carpenter and Noble, who attempted to explain them by "expectant attention." In a thesis read before the Faculty of Medicine of Paris in 1878, Mr. Oscar Jennings made himself the champion of the ideas expressed by these English writers.

As to what relates to the magnet itself we are going to show, summarily, the arguments upon which are based its physiological action and its therapeutical use.

The action of the magnet, among effects produced by other physical agents of which we have spoken (plates of different metals, electricity, vibrations of the diapason), presents itself in a more surprising way, and, indeed, in a way *à priori* prone to excite incredulity. The application is not direct. The magnet is not placed in contact with the skin of the subject upon whom the experiment is tried, as it is necessary to do with other metalic plates, its action being exerted at a distance. It is sufficient to influence the organism, and produce the same effects as other metals, to place the magnetized bar at a distance of one to two centimetres from the portion of the body upon which we wish to make an impression. All the experiments at the Salpêtrière have been made with these conditions. The effects produced in these cases were not attributed to the action of the metal, and belong properly to magnetism itself.

The magnet, let us say, acts in some way on the organism when in these special morbid conditions. Before speaking of the facts which prove peremptorily that this action exists, can we not, if not explain it, at least conceive of the possibility of such an effect. The action of

[1] Among the authors who have given attention to the action of the magnet in medicine, we may cite: Pliny the Younger, Paracelsus, Albert the Great, the older Hell (1770), Mesmer (1779), Andry and Thouret (1780), Becker (1829).

[2] See *La Nature*, Feb. 17, 1877.

physical forces upon biological phenomena has long since been admitted; who does not recognize the importance of heat, of light, and of electricity upon the vital manifestations? In medicine electricity, under its different forms, is daily employed in the treatment of a multitude of diseases. Why refuse to one physical force that which we accord to all the others? Why, if all physical agents are only varied forms of the same force, should they not all exert an action upon the organism in a measure different for each of them? And then why should not magnetism, which possesses in such a high degree this very singular property of influence at a distance, be able in physiological order to produce analogous effects?

If from conjectures we pass to the consideration of facts, we are forcibly convinced that this physiological action of magnetism at a distance does truly exist.

In physics, experiment leads to a, in some degree, tangible result; it is undeniable; it forces itself upon us. Bring the poles of a magnetized bar near to some iron filings and you have the conditions for the experiment; the iron is attracted and there is the result. Discussions may arise upon the theory, upon the interpretation of the fact, but the fact itself is always present. Furthermore, as it is easy to appreciate exactly all of the circumstances of the experiment, we are certain that with the same conditions given, we shall always obtain the same result. In a word, the experiment can be easily repeated. In physiology experiment is surrounded by the greatest difficulties, but the result is neither less significant nor less certain. As regards experiments made with the magnet, it can be shown that they fulfill all the conditions of certainty of physical experiments.

In the first place it is necessary that the application be well done; that is to say, that the magnet ought to be in good condition and properly placed. It is not necessary that the magnet should be very large, nor endowed with very energetic properties; it is sufficient if the magnetic force exist in an appreciable degree. The experiment has often been attempted with false magnets, that is to say, with bars or horseshoes of variable substance, zinc, copper, wood, etc., not possessing any magnetic action, but having all the appearances of true magnets. In these cases the experiment has always given negative results. Likewise, in making use of the electro-magnet, the action upon the organism takes place only when the established current gives to the soft iron its magnetic properties. It is necessary also that the magnet should be properly applied; the poles alone acting, the neutral portion should remain absolutely without effect. This is easily accomplished when using the magnet in the form of a horseshoe and by presenting it successively by its open and its closed side. A patient has his eyes bandaged; the magnet is applied to the region of the back, in such a manner that he shall have no knowledge of its position, and physiological phenomena always follow such application of the poles, and never follow the application at the neutral line.

But is it easy to verify the result obtained? How does the magnet work? How is its action demonstrated? Does it not depend, one will say, upon phenomena of sensibility which are purely subjective and appreciable with great difficulty to the experimenter, who is obliged to trust to the word of his patient? I will reply at once that it is sufficient to have assisted at a single one of the experiments at the Salpêtrière to be convinced that these phenomena, subjective it is true, can be easily rendered objective. A large needle made to pass unawares through the flesh of the subject whose eyes are kept carefully bandaged, shows in an absolutely objective way the profound anæsthesia which the parts have attained. But the phenomena of sensibility are not the only ones produced; the magnet has an influence upon temperature, as the thermometer distinctly shows.[1] It acts also on the *motilité* of the parts to which it is applied, provoking contractions of an intensity and of a duration which removes all suspicion of simulation. The physician must be a mere novice who could mistake a prolonged and voluntary contraction for a true contracture. Now, the magnet produces in certain cases true contractures. (*La Nature.*)

DR. P. RICHET.

(*To be continued.*)

[1]M. Broca presented to the Academy of Medicine, at the Session of February 7, a note from Dr. Henrot (of Reims) upon the action of the magnet in hemihypothermia. He states that the application of three magnets to the cold limb raised its temperature from 1.8° to 2.3° in twenty minutes, at the same time lowering the temperature of the normal member two-tenths of a degree.

NOTES AND QUERIES.

To the Editor of Science:

[1.] In the 3d. July number of SCIENCE, under "Notes and Queries," J. H. G desires "reliable information concerning the Tuckahœ."

I will tell him what I know concerning it, in regular sequence with his questions:

1st. What is its geographical distribution?

I know, from North Carolina to Texas, along the gulf States—presumably elsewhere also.

2nd. What is the nature of its growth and production?

An underground fungus or root, growing under the surface like the truffle. Belongs to the gasteromycetous fungi, according to Lindley and Fries.

3d. Its former use and preparation?

Used as bread, by roasting in the ashes, both by Indians and negroes.

4th. In what soil found?

Sandy or loamy surface with sub-soil of clay.

5th. What authors have mentioned it?

Dr. McBride and F. P. Porcher of South Carolina; Clayton, Le Conte, M. J. Berkeley, Fries & Lindley.

6th. By what botanical name is it known?

Lycoperdon solidum.

7th. Has it any medicinal virtues?

I know of none.

It is considered very nutritious hence its name *tuckahœ*, which is Indian for *bread*, and is highly prized by the negroes, who eat it to this day.

There is a district of country in the eastern counties of North Carolina called Tuckahœ from the abundance of these subterraneous bodies.

Although the Tuckahœ has been placed among the fungi, yet there is considerable doubt as to its true position in the vegetable kingdom.

Analysis shows it to consist almost entirely of pectic acid, which would seem to remove it from the fungi, and yet the entire absence of vascular or cellular structure of bark, etc., would seem to remove it equally as far from the phænogams.

I hope these few *enseignments* may help J. H. G., and that he will favor us with the results of his further researches.

MRS. M. J. YOUNG,

HOUSTON, TEXAS, July 19th.

THE Rev. W. Cowell Brown, Wesleyan minister of Sheffield, has patented an invention which appears to be a simple and practical means of lessening the number of deaths by drowning. A chemical preparation is inserted in a portion of the coat, waistcoat or dress. It does not add to the weight, or in any way alter the appearance of the garment. The preparation is inserted between the lining and the cloth; in the case of a coat, it is placed on each side of the coat and up the back. The moment a man falls into the water the coat becomes inflated, and he cannot keep his head under the waves. The invention has already been thoroughly tested, and it is stated that it will sustain a person in the water as long as he can possibly endure the exposure, say forty-five or fifty hours.

SATURDAY, AUGUST 7, 1880.

BIRD FURNITURE.

BY DR. J. B. HOLDER, ASST. SUPT. AM. MUSEUM.

It is fair to say that, hitherto, no serious effort has been directed toward the proper furnishing of museum cabinets. This is especially the case with reference to Ornithology. Excellent specimens, often rare, all most valuable, and many special pets, highly prized, are placed with such surroundings and accessories as to very much detract from the pleasing effect they are capable of producing. This is so patent that any intelligent observer must have noticed the incongruity, though the cause may not be clear to him.

This subject will bear a very much fuller consideration than we can now give it. We will, however, present the results of a deliberate and careful study of certain *desiderata* in this connection. The trustees of the American Museum of Natural History in Central Park, New York, in view of the great value and scientific importance of the Prince Maximillian collection of birds, owned by the museum, determined to have them mounted in a manner commensurate with their worth.

A large collection of skins of North American birds presented by Mr. Elliott, was also placed in the hands of the taxidermist. To be in keeping with the excellent work sure to come from Mr. Bell, the matter of

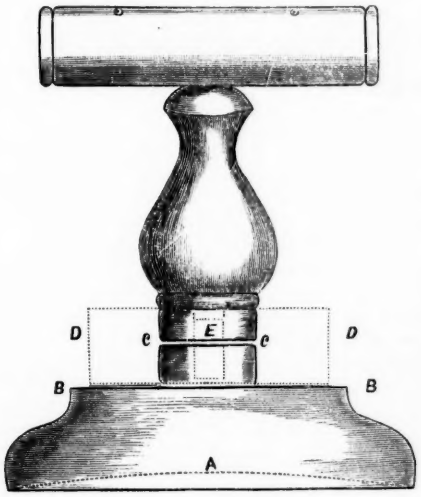

Fig. 1.

perches and stands became of the first importance. In large museums specimens are frequently moved to allow of more varied views, or to give place to others near them: it is desirable to exhibit one bird facing, another sidewise, the next with its back in front; this involves a change or removal of label.

To simplify this, as the result of our experiments, we refer to the figure of the bird perch. The wood is of the plainest straight-grained mahogany, handsomely polished. Though somewhat more expensive, it is regarded economical to use such furniture, as in a large museum it is manifestly desirable to avoid any future overhauling, or what householders call "spring cleaning." The polished stands and perches only require the occasional use of a feather duster. Birds once mounted in this manner require nothing further, and remain intact for all time. The base of the perch is hollowed, as indicated by dotted lines at "A." This is to allow labels or written notes to be concealed safely beneath for reference.

The top of the base "B.B." is, practically, a tablet, upon which the label rests and rotates. The upright or column presents an appearance of completeness after the label is mounted; the bead just under which the label works forming the base, all below being out of sight.

Fig. 2.

"E" represents a bit of tin which embraces the label in front on its upper edge, and lies behind and in contact with it. A slender copper wire is passed around the tin, and is then compressed into the groove which surrounds the upright at "C.C." This wire is twisted behind the upright, and left projecting sufficiently to allow it to be held by the thumb-nail while rotating the label upon the tablet, "B.B." It will be readily seen that this movement is easily made, and constitutes the chief point of interest in the perch. We think also that the proportions of the perch, the manifest harmony of parts, the fitting relations of label and tablet, each designed for the other in due proportions, may be regarded as improvements more or less in advance of the old methods.

Another group of birds than those that perch require flat stands; as ducks, walking birds, etc.

Our second illustration exhibits a device, very simple, yet suitable. It is desirable to place the label upon the stand, so that it may be removed or its

locality changed readily. The block, as seen in the figure, has a bead on each edge; these beads are grooved to admit the label, which is of stiff parchment paper. The upper groove is deepest, so that the upper edge of the label may be pressed into it sufficiently to allow the lower edge to drop into the lower groove. It will be seen that we then have a label free to move, the entire periphery giving the utmost freedom for exhibiting every aspect of the specimen. Of course these blocks may be made with upright sides. Though the labels are more expensive for the bevelled ones, the latter have a more pleasing appearance.

COMPETITION BETWEEN THE ANILINE AND MADDER DYES.

BY A. S. MACRAE.

As these dyes are globularly used to the extent of some one hundred million dollars per annum; as they are as well known to the manufacturers of New England as to the horse-hide colorers of Japan, it may be interesting to inquire what effects, in *esse* and *posse* the one is having upon the other in commercial value. And as the market price invariably depends upon supply and demand, the source of the former must be examined into that the estimate of the latter may lead to judicious deductions.

Previous to the modern use of the above, indigo, cochineal, and the vegetable or wood dyes were altogether in vogue, and the inestimable appreciation of the indigo was primarily the cause which led to the discovery of aniline. The coloring matter of indigo has long been technically known as anil, and the manner in which it gave the name to aniline, has perhaps never been published before this present article. The botanist had ever been puzzled to know whence came the coloring matter of the indigo plant. Where it was indigenous the dyeing matter was inherent; but although the plant flourished almost anywhere in tropical climates, it invariably lost its color yielding power on this transportation! How was this? The botanist had to appeal to the chemist for explanation. Investigation demonstrated that the anil or coloring matter was solely due to the subsoil over which the indigo plant fructified, and that apart from this metaliferous or possibly bituminous earth, the coloring idiosyncrasy was lost. It will thus be seen that the article cannot be produced at will, but only where it and the soils are indigenous. However much this certainty baffled the botanist, it only set the chemist a-thinking. His analysis and synthesis showed beyond cavil, that anil, pure and simple, was neither more nor less than a hydro-carbonic compound, and that amongst some of these artificially produced compounds, anil, otherwise than the anil of indigo, might yet be discovered. The cheapest object for this research naturally suggested itself, and common coal-tar—the refuse of gas works—presented itself as the most economic basis of naphtha, and the matrix of an abundant hydro-carbon. It would be irrelevant here to trace the success which crowned the chemists' efforts to produce anil, or as it was now called, aniline, from this once—but now no longer so—rejected filth. But one portion of the discovery must be referred to, not only in demonstrating the discoverers' wonderful patience, but as proof of the capricious supply of this marvellous product. Coal tar, then, yields naphtha; naphtha, benzole; benzole, nitro-benzole; nitro-benzole, aniline. When the naphtha was first distilled from coal tar, no benzole was discovered in it, or, if it was discovered, in such small quantities as to defy remunerative production. But the trace was there, and as most auriferous deposits are discovered by traces, these said traces were pursued until the golden goal was scientifically and successfully attained. When the naptha was distilled by different temperatures, it was found that benzole was produced at one temperature that was smothered at another, and that by grading the distillations actual benzole could be eliminated in paying quantities! From this moment common coal-tar became the matrix of those valuable aniline dyes, which under the names of roseine, aniline reds and crimsons, Nicholson's blues, Humbolts, mauves, magentas, Bismark browns, oranges, iodine greens, purples, magdalas, violets, greens, phosphines, etc., have astonished the world for the last twenty years. Nearly all the dazzling colors worn now-a-days, that dim the sun and flaunt the eyes, are derived from the very cheapest of bases named, yet have arrived at such a value in the manipulation, that prices run from $2 to $30 a pound and in some cases even $6 an ounce.

At the period of these discoveries, madder had largely superceded indigo, cochineal and other dyes, and at its producable price was certainly the most economic dyeing product extant. Madder is neither more nor less than the ordinary madder root ground, a root capable of cultivation to an unlimited extent. Turkey in Asia, Italy, France, Spain, Holland, and Naples produced it in enormous quantities and British India soon followed suit. The importations into Great Britain at one time amounted to 50,000 tons, and at least a similar quantity was consumed in the countries of production. Unknown as madder may be by that nomenclature, every housewife knows it under the appellation of the "Turkey Red," the name manufacturers gave to their prints dyed by this article. Some idea of its consumption even in America may be given, when it is stated that the writer of this article saw some 500 tons of this madder in the manufactory of A. & W. Sprague & Co., of Providence, R. I., when he visited those works a few years ago.

If then aniline is used by the *pound* where madder is used by the *ton*, it may well be asked by merchants, manufacturers and dyers, what will be the effect of the competition between them? the one the limited production of human manipulation, the other the unlimited production of cultivated nature. We will examine the question.

"Every dog has its day," and in the day of aniline

there was but one opinion, that it would sweep every other dye out of the vat-house. Not only was its application so simple, requiring solvents instead of mordants, but at the price, and especially at the price then current for all dyes, it was the cheapest, with given results. A cosmopolitan demand at once set in, therefore, for anilines, a demand which not only enhanced figures to famine prices, but which was far beyond the possibility of supply. That supply depended on coal tar; coal tar depended upon gas works; gas works, after all, are of limited number all over the world—*ergo*, the aniline supply could be but limited. As madder fell into a state of almost desuetude, prices naturally depreciated, until from an average of twelve cents a pound, it is not now worth two cents. Thus, as aniline became scarce, madder became cheap, and manufacturers were enabled to pit their "Turkey Reds" in the shape of Pompadour prints and their like, at prices the very best informed anilinists, or anybody else, never dreamt of. And this brings us to the issue.

We cannot now see, whatever we foresaw in bygone days, that madder and its derivatives, have anything at all to fear from aniline and its beautiful eliminates. As circumstances alter cases, so the position of the two chief dyes are equalized by the extent of the supply and the restrictions of demand. Aniline can not be produced *ad libitum*, madder can. Almost unlimited high prices will always be given for the former; but the latter, experience shows us for the first time, can be grown for almost unlimited low prices. The rich and the poor consumers can thus be well served; but madders go with the poor and therefore the popular prices of both may, nay they will, fluctuate as markets may dictate; but the fear that aniline will end in the supercession of madder is, we think, entirely groundless. The madder "day" is imminent, if not actual now-a-days, and wherever we go its "hues" are more prominent than those of its great competitors.

THE influence of magnetisation on the tenacity of iron has been lately studied by Signor Piazzoli. Iron wires were hung between two hooks and ruptured by pouring water into a vessel suspended from them. They were about 350 mm. long, and were inclosed in a spiral with four windings one over another, which were either all traversed by a current in one direction, or two by a current in one direction, and two by an equal opposite current, so that in both cases the wires were equally strongly heated by the spiral; but in one case they were magnetised, in the other not. The weights required to break wires annealed in charcoal——weight of one metre, G = 0·299—were, during magnetisation, P = 1260-1306; without magnetisation, P′ = 1213-1270. In the case of wires annealed in carbonic oxide—where G = 0·46 g·—P = 1732·4-1742·7; P′ = 1703·62-1719·87. In the case of wires annealed in hydrogen P = 1289·5-1310·1; P′=1263-1299·7. In each separate series accordingly the difference, P — P′ was frequently less than the difference between the highest and lowest weights required for rupture of apparently identical wires; still, the mean values in each of the fourteen series were from about 1 to 3 per cent greater for the magnetised than for the unmagnetised wires, showing that the tenacity of iron increases on magnetisation.

DEGENERATION.

BY ALFRED R. WALLACE.

Degeneration causes an organism to become more simple in structure, in adaptation to less varied and less complex conditions of life. "Any new set of conditions occurring to an animal which render its food and safety very easily attained, seem to lead as a rule to degeneration; just as an active healthy man degenerates when he becomes suddenly possessed of a fortune; or as Rome degenerated when possessed of the riches of the ancient world. The habit of the parasitism clearly acts upon animal organisation in this way. Let the parasitic life once be secured, and away go legs, jaws, eyes and ears; the active and highly-gifted crab, insect, or annellid may become a mere sac, absorbing nourishment and laying eggs."

We see incipient cases of degeneration in the loss of limbs of the serpentiform lizards and the pisciform mammals; the loss of eyes in the inhabitants of caverns and in some earth-burrowers; the loss of wings in the Apteryx and of toes in the horse; and, still more curious, the loss of the power of feeding themselves in some slave-holding ants. More pronounced cases are those of the barnacles—degenerated crustacea, and the mites—degenerate spiders; while we reach the climax of the process in Ascidians—degenerate vertebrates, and such mere living sacs as the parasitic Sacculina and Lernæocera, which are degenerated crustaceans. Not only such lesser groups as the above, but whole orders may be the result of degeneration. Such are the headless bivalve mollusca known as Lamellibranchs, which are believed to have degenerated from the head-bearing active cuttle-fish type; while the Polyzoa or Moss-polyps stand in the same relation to the higher Mollusca as do the Ascidians to the higher Vertebrates.

While discarding the hypothesis that all savages are the descendants of more civilized races, Prof. Lankester yet admits the application of his principle to explain the condition of some of the most barbarous races—"such as the Fuegians, the Bushmen, and even the Australians. They exhibit evidence of being descended from ancestors more cultivated than themselves." He even applies it to the higher races in intellectual matters, and asks: "Does the reason of the average man of civilized Europe stand out clearly as an evidence of progress when compared with that of the men of bygone ages? Are all the inventions and figments of human superstition and folly, the self-inflicted torturing of mind, the reiterated substitution of wrong for right, and of falsehood for truth, which disfigure our modern civilization—are these evidence of progress? In such respects we have at least reason to fear that we may be degenerate. It is possible for us—just as the Ascidian throws away its tail and its eyes and sinks into a quiescent state of inferiority—to reject the good gift of reason with which every child is born, and to degenerate into a contented life of material enjoyment accompanied by ignorance and superstition."

This is very suggestive; but we may, I think, draw a yet higher and deeper teaching from the phenomena of degeneration. We seem to learn from it the absolute necessity of labor and effort, of struggle and difficulty, of discomfort and pain, as the condition of all progress, whether physical or mental, and that the lower the organism the more need there is of these ever-present stimuli, not only to effect progress, but to avoid retrogression. And if so, does not this afford us the nearest attainable solution of the great problem of the origin of evil? What we call evil is the *essential* condition of progress in the lower stages of the development of conscious organisms, and will only cease when the mind has become so thoroughly healthy, so well balanced, and so highly organized, that the happiness derived from mental activity, moral harmony, and the social affections, will itself be a sufficient stimulus to higher progress and to the attainment of a more perfect life.

For numerous instructive details connected with degenerated animals we refer our readers to the work itself—truly a small book on a great subject, and one which discusses matters of the deepest interest, alike to the naturalist and the philosopher.—*Nature*.

PHYSIOLOGY OF THE FRESH WATER MEDUSA.

The structure of this remarkable animal has already been investigated and described by Professors Allman and Lankester, with the result of showing that, although constituting a new genus, it is in all respects a true Medusa. After the publication of their papers I began to work out the physiology of the new form, and the following are the results which so far I have obtained.

The natural movements of the Medusa precisely resemble those of its marine congeners. More particularly, these movements resemble those of the marine species which do not swim continuously, but indulge in frequent pauses. In water at the temperature of that in the Victoria Lily-house (85° F.) the pauses are frequent, and the rate of the rhythm irregular—suddenly quickening and suddenly slowing even during the same bout, which has the effect of giving an almost intelligent appearance to the movements. This is especially the case with young specimens. In colder water (65° to 75°) the movements are more regular and sustained; so that, guided by the analogy furnished by my experiments on the marine forms, I infer that the temperature of the natural habitat of this Medusa cannot be so high as that of the water in the Victoria Lily-house. In water at that temperature the rate of the rhythm is enormously high, sometimes rising to three pulsations per second. But by progressively cooling the water, this rate may be progressively lowered, just as in the case of the marine species; and in water at 65° the maximum rate that I have observed is eighty pulsations per minute. As the temperature at which the greatest activity is displayed by the freshwater species is a temperature so high as to be fatal to all the marine species which I have observed, the effects of cooling are of course only parallel in the two cases when the effects of a series of high temperatures in the one case are compared with those of a series of lower temperatures in the other. Similarly, while a temperature of 70° is fatal to all the species of marine Medusæ which I have examined, it is only a temperature of 100 degrees that is fatal to the freshwater species. Lastly, while the marine species will endure any degree of cold without loss of life, such is not the case with the freshwater species. Marine Medusæ, after having been frozen solid, will, when gradually thawed out, again resume their swimming movements; but this freshwater Medusa is completely destroyed by freezing. Upon being thawed out, the animal is seen to have shrunk into a tiny ball, and it never again recovers either its life or its shape.

The animal seeks the sunlight. If one end of the tank is shaded, all the Medusæ congregate at the end which remains unshaded. Moreover, during the daytime they swim about at the surface of the water; but when the sun goes down they subside, and can no longer be seen. In all these habits they resemble many of the sea-water species. They are themselves non-luminous.

I have tried on about a dozen specimens the effect of excising the margin of the nectocalyx. In the case of all the specimens thus operated upon, the result was the same, and corresponded precisely with that which I have obtained in the case of marine species. That is to say, the operation produces immediate, total, and permanent paralysis of the nectocalyx, while the severed margin continues to pulsate for two or three days. The excitability of a nectocalyx thus mutilated persists for a day or two, and then gradually dies out—thus also resembling the case of the marine naked-eyed Medusæ. More particularly, this excitability resembles that of those marine species which sometimes respond to a single stimulation with two or three successive contractions.

A point of specially physiological interest may be here noticed. In its unmutilated state the freshwater Medusa exhibits the power of localizing with its manubrium a seat of stimulation situated in the bell. That is to say, when a part of the bell is nipped with the forceps, or otherwise irritated, the free end of the manubrium is moved over and applied to the part irritated. So far, the movement of localization is precisely similar to that which I have previously described as occurring in *Tiaropsis indicans* (*Phil. Trans.*, vol. clxvii.) But further than this, I find a curious difference. For while in *T. indicans* these movements of localization continue unimpaired after the margin of the bell has been removed, and will be ineffectually attempted even after the bell is almost entirely cut away from its connections with the manubrium; in the freshwater Medusa these movements of localization cease after the extreme margin of the bell has been removed. For some reason or another the integrity of the margin here seems to be necessary for exciting the manubrium to perform its movements of localization. It is clear that this reason must either be that the margin contains the nerve-centres which preside over these localizing movements of the manubrium, or much more probably, that it contains some peripheral nervous structures which are alone capable of transmitting to the manubrium a stimulus adequate to evoke the movements of localization. In its unmutilated state this Medusa is at intervals perpetually applying the extremity of its manubrium to one part or another of the margin of the bell, the part of the margin touched always bending in to meet the approaching extremity of the manubrium. In some cases it can be seen that the object of this co-ordinated movement is to allow the extremity of the manubrium—*i.e.*, the mouth of the animal—to pick off a small particle of food that has become entangled in the marginal tentacles. It is therefore not improbable that in *all* cases this is the object of such movements, although in most cases the particle which is caught by the tentacles is too small to be seen with the naked eye. As it is thus no doubt a matter of great importance in the economy of this Medusa that its marginal tentacles should be very sensitive to contact with minute particles, so that a very slight stimulus applied to them should start the co-ordinated movements of localization, it is not surprising that the tentacular rim should present nerve-endings so far sensitive that only by their excitation can the reflex mechanism be thrown into action. But if such is the explanation in this case, it is curious that in *Tiaropsis indicans* every part of the bell should be equally capable of yielding a stimulus to a precisely similar reflex action.

In pursuance of this point I tried the experiment of cutting off *portions* of the margin, and stimulating the bell *above the portions of the margin which I had removed.* I found that in this case the manubrium did not remain passive as it did when the *whole* margin of the bell was removed; but that it made ineffectual efforts to find the offending body, and in doing so always touched some part of the margin which was still unmutilated. I can only explain this fact by supposing that the stimulus supplied to the mutilated part is spread over the bell, and falsely referred by the manubrium to some part of the sensitive—*i.e.*, unmutilated—margin.

But to complete this account of the localising movements it is necessary to state one additional fact which, for the sake of clearness, I have hitherto omitted. If any one of the four radial tubes is irritated, the manubrium will correctly localise the seat of irritation, whether or not the margin of the bell has been previously removed. This greater case, so to speak, of localising stimuli in the course of the radial tubes rather than anywhere else in the umbrella except the margin, corresponds with what I found to be the case in *T. indicans*, and probably has a direct reference to the distribution of the principal nerve-tracts.

On the whole, therefore, contrasting this case of localisation with the closely parallel case presented by *T. indicans*, I should say that the two chiefly differ in the freshwater Medusa, even when unmutilated, not being able to localise so promptly or so certainly: and in the localisation being only performed with reference to the margin and radial tubes, instead of with reference to the whole excitable surface of the animal.

All marine Medusæ are very intolerant of fresh water, and therefore as the fresh water species most presumably have had marine ancestors,[1] it seemed an interesting question to determine how far this species would prove tolerant of sea water. For the sake of comparison I shall first briefly describe the effects of fresh water upon the marine species.[2] If a naked eyed Medusa which is swimming actively in sea water is suddenly transferred to fresh water, it will instantaneously collapse, become motionless, and sink to the

[1] Looking to the enormous number of marine species of Medusæ, it is much more probable that the freshwater species were derived from them, than that they were derived from freshwater ancestry.

[2] For full account, see *Phil. Trans.*, vol. clxvii., pp. 744-745.

bottom of the containing vessel. There it will remain motionless until it dies; but if it be again transferred to sea water it will recover, provided that its exposure to the fresh water has not been of too long duration. I have never known a naked-eyed Medusa survive an exposure of fifteen minutes: but they may survive an exposure of ten, and generally survive an exposure of five. But although they thus continue to live for an indefinite time, their vigor is conspicuously and permanently impaired. While in the fresh water irritability persists for a short time after spontaneity has ceased, and the manubrium and tentacles are strongly retracted.

Turning now to the case of the freshwater species, when first it is dropped into sea water at 85° there is no change in its movements for about fifteen seconds, although the tentacles may be retracted. But then, or a few seconds later, there generally occurs a series of two or three tonic spasms separated from one another by an interval of a few seconds. During the next half minute the ordinary contractions become progressively weaker, until they fade away into mere twitching convulsions, which affect different parts of the bell irregularly. After about a minute from the time of the first immersion all movement ceases, the bell remaining passive in partial systole. There is now no vestige of irritability. If transferred to fresh water after five minutes exposure, there immediately supervenes a strong and persistent tonic spasm, resembling rigor mortis, and the animal remains motionless for about twenty minutes. Slight twitching contractions then begin to display themselves, which, however, do not affect the whole bell, but occur partially. The tonic spasm continues progressively to increase in severity, and gives the outline of the margin a very irregular form; the twitching contractions become weaker and less frequent, till at last they altogether die away. Irritability, however, still continues for a time—a nip with the forceps being followed by a bout of rhythmical contractions. Death occurs in several hours in strong and irregular systole.

If the exposure to sea water has only lasted two minutes, a similar series of phenomena are presented, except that the spontaneous twitching movements supervene in much less time than twenty minutes. But an exposure of even one minute may determine a fatal result a few hours after the Medusa has been restored to fresh water.

Contact with sea water causes an opalescence and essential disintegration of the tissues, which precisely resemble the effects of fresh water upon the marine Medusæ. When immersed in sea water this Medusa floats upon the surface, owing to its smaller specific gravity.

In diluted sea water (50 per cent) the preliminary tonic spasms do not occur, but all the other phases are the same, though extended through a longer period. In sea water still more diluted (1 in 4 or 6) there is a gradual loss of spontaneity, till all movement ceases, shortly after which irritability also disappears; manubrium and tentacles expanded. After an hour's continued exposure intense rigor mortis slowly and progressively develops itself, so that at last the bell has shrivelled almost to nothing. An exposure of a few minutes to this strength places the animal past recovery when restored to fresh water. In still weaker mixtures (1 in 8, or 1 in ten) spontaneity persists for a long time; but the animal gradually becomes less and less energetic, till at last it will only move in a bout of feeble pulsations when irritated. In still weaker solutions (1 in 12, or 1 in 15) spontaneity continues for hours, and in solutions of from 1 in 15 to 1 in 18 the Medusa will swim about for days.

It will be seen from this account that the freshwater Medusa is even more intolerant of sea water than are the marine species of freshwater. Moreover the freshwater Medusa is beyond all comparison more intolerant of sea water than are the marine species of brine. For I have previously found that the marine species will survive many hours' immersion in a saturated solution of salt. While in such a solution they are motionless, with manubrium and tentacles relaxed, so resembling the freshwater Medusa shortly after being immersed in a mixture of 1 part sea water to 5 of fresh; but there is the great difference that while this small amount of salt is very quickly fatal to the fresh-water species, the large addition of salt exerts no permanently deleterious influence on the marine species.

We have thus altogether a curious set of cross relations. It would appear that a much less profound physiological change would be required to transmute a sea-water jelly-fish into a jelly-fish adapted to inhabit brine, than would be required to enable it to inhabit fresh water. Yet the latter is the direction in which the modification has taken place, and taken place so completely that sea water is now more poisonous to the modified species than is fresh water to the unmodified. There can be no doubt that the modification was gradual—probably brought about by the ancestors of the freshwater Medusa penetrating higher and higher through the brackish waters of estuaries into the fresh water of rivers—and it would I think be hard to point to a more remarkable case of profound physiological modification in adaptation to changed conditions of life. If an animal so exceedingly intolerant of fresh water as is a marine jelly-fish may yet have all its tissues changed so as to adapt them to thrive in fresh water, and even die after an exposure of one minute to their ancestral element, assuredly we can see no reason why any animal in earth or sea or anywhere else may not in time become fitted to change its element.

GEORGE J. ROMANES.

A NEW GENUS OF RHINOCERONTIDÆ.

While the genus *Aphelops* must be regarded as the direct ancestor of the recent rhinoceroses with canine and incisor teeth, now confined to Asia and the Islands, the ancestral genus of the African forms and their extinct congeners, which are without the teeth named, is less known. It can now be shown that the missing genus inhabited North America, and that, like *Aphelops*, it is hornless. It may be named and characterized as follows: *Peraceras*, Cope; superior dentition; I. 0; C. 0; P-m. 4; M. 3; nasal bones weak, hornless.

This genus is established on a new basis recently discovered by Mr. R. H. Hazard, in the Loup Fork formation of Nebraska, which may be called *Peraceras superciliosus*. It is founded on a nearly perfect skull, which lacks the lower jaw. Its size is about that of the Indian rhinoceros. It is narrowed anteriorly, but is very wide between the orbits. Posterior to these it contracts rapidly, and rises to a rather elevated occiput. Saggital crest narrow; a prominent angle above each orbit. The premaxillary bone is narrow and weak. The nasal notch extends to above the middle of the third superior premolar. The occiput is rectangular in outline, with truncate summit. Its surface above is concave, divided by a strong median crest; lower down a vertical groove intersects its lateral border. The crests of the molar teeth are rather simple, and the posterior notch is soon isolated on attrition. Wear also isolates an external median fossa of the second premolar. Length of skull from end of premaxillary bone to condyles, M. 700; length of alveolar border of premaxillary, .025; length of molar series, .315; length of three true molars, .160; width of crown of second true molar at base, .075; superciliary width, .255.

This species is nearest to the *Peraceras malacorhinus*, a species which I formerly referred to *Aphelops*, but which I have little doubt belongs to the present genus. It differs from *P. superciliosus* as follows: In the latter species the front is wider, and is plane or concave, not convex; the superior edge of the maxillary is not wide and incurved, and has not the oblique ridges; the infraorbital foramen consequently has a more lateral opening. The nareal notch does not extend so far posteriorly by the one and a half molar teeth. The occiput is wider, is divided by a median crest not found in *P. malacorhinus*, and has the vertical lateral grooves much shorter. The acute supraorbital angle is not seen in the *P. malacorhinus*.

The rhinoceroses of the Loup Fork formation whose generic position can now be ascertained, are the following: *Paraceras malacorhinus; P. superciliosus; Aphelops meridianus; A. Negalodus; A. fossiger.*—*Am. Naturalist.*